

FACTS AND CONCLUSIONS

AS TO THE USE OF

ALCOHOLIC STIMULANTS IN TYPHUS FEVER

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THE following remarks are so entirely the result of practical experience, and so little dependent upon any of the current theories in regard to the physiological and therapeutical action of alcoholic stimulants, that it seems scarcely necessary to disclaim, at the outset, any exclusive or extreme views on the part of their author. There is one thing, however, which he feels bound most emphatically to disclaim, and that is anything approaching a spirit of hostile or ungenerous criticism as regards the views or practices of those who may differ from him. In saying, as he desires to do with all seriousness, that alcoholic stimulants are often administered, even in the hands of accomplished and thoughtful physicians, with far too great freedom in many kinds of acute disease, he is quite well aware that the statement is liable to misinterpretation, as being in effect an attack upon principles or modes of practice held in perfect good faith by persons from whom he would not willingly differ. But he trusts that the whole tenor of the remarks which follow will be such as to satisfy every one who hears them:—1st, That the author has no personal object in view in his strictures; 2nd, That the subject has been long before his

mind, and is not approached without due consideration; 3rd, That it has not been approached from the side of a sweeping general conclusion, but rather in the way of cautious and deliberate experiment, worked out with many securities taken against error and prejudice, over a long series of years.

It is, in fact, well-known that the conscientious opinions of many distinguished physicians, for many years past, have been in favour of very considerable doses of stimulants, given almost as a matter of routine in many acute diseases from an early period of the disease, and continued throughout the period of danger, and often some way into that of convalescence. The name of the late Dr. Todd has often been used in relation to this practice, and beyond all doubt Dr. Todd has been its ablest and most extreme defender of late years, so far as regards written and published statements in its favour. But my impression is that Dr. Todd was rather the expositor of a general tendency in medical practice than the deviser of anything really new and striking in theory in this matter; he was borne (willingly borne, it may be) upon a tide of opinion, which, no doubt, he placed in an extreme point of view, and justified by most able and plausible arguments; but he was in no sense whatever the originator of the doctrine or of the practice he followed with respect to alcoholic stimulants in acute disease. The frenzy of approval with which his views were accepted in some quarters was the indication of their being received as the vindication of pre-existing opinions and practices rather than as the exploration of an undiscovered country; and it is known to most well-informed practitioners that, long before Dr. Todd adopted the routine of stimulation he afterwards advocated, similar practices had been widely diffused throughout the profession, both in the metropolis and elsewhere. In particular, as regards typhus fever, doctrines not very dissimilar from those of Dr. Todd were taught as early as 1826 by my much-revered teacher, the late Dr. Alison; with this difference, however, from the doctrines and practices of Dr. Todd, that the latter physician was both more extreme and more indiscriminate in his use of stimulants than the former, Dr. Alison relying upon a specific *observed* change of type in disease as the justification of his practice; while Dr. Todd, on the other hand,

advocated the lavish use of stimulants in almost all acute diseases, and in almost all stages of them, with but little regard to individual differences of age, sex, &c., and without any respect to epidemic peculiarities, or any qualification on account of the previous habits of the patient.* I believe, however, that all the old pupils of Dr. Alison will remember his constant instruction to the effect that, in typhus fever, and other diseases of a typhoid character, stimulants were always to be administered from an early period, without waiting for symptoms of debility, or for any other special indication, except the ascertained nature and tendencies of the disease; and, in point of fact, the administration of wine and spirits by Dr. Alison, in such diseases, must be well remembered by all who observed it, as being practically of the nature of a routine, not indeed nearly so extreme or unqualified as that of Dr. Todd, but essentially of the same character, the use of stimulants being commenced almost invariably before the end of the first week, and sometimes as early as the second or third day, and continued throughout the disease in liberal doses, increased according to the violence of the symptoms. There can be no doubt that this practice of Dr. Alison, carefully followed out by his pupils Drs. Graves and Stokes in Ireland, and by others all over the world, had a powerful influence in breaking up the old bad routine of depletion in typhus fever, which was almost universal in the earlier part of the present century; but it seems to me hardly less probable that to the exaggeration of Dr. Alison's rules of practice we owe, indirectly, the more extreme doctrines of these latter days, as regards the use of stimulants in almost all acute diseases. But be this as it may, I propose, in the present paper, to avoid all disquisitions of a theoretical kind, and to allow my own opinions to emerge from a simple statement of facts, which will, I trust, be considered not unworthy of attention, even should the inferences I draw from them be regarded as not fully proved.

There are many reasons for raising the question of the

* I infer this from the fact that some of the most considerable doses noticed in Dr. Todd's book, are given in cases of young women—*c. g.* to a girl 17 years of age, 6 drachms of brandy every hour, (Case LXIV); and this without any statement in regard to previous habits.

practical limits of the administration of stimulants, in the first instance, in the case of typhus fever. For, whatever differences of opinion may exist on the subject in general, no one can refuse to admit that typhus fever is the disease in which the practice of stimulation has been most largely followed, and in which it has found the most general acceptance, as being in accordance both with rational theory and actual experience of facts. Moreover, typhus fever, as marked by the eruption, is a remarkably well-defined disease, in which, with proper securities against error, there is likely to be but little difference between the diagnosis of one well-informed hospital physician and another; and besides, typhus has constituted by far the largest proportion of all the fevers treated in hospital in Glasgow of late years. Further, the arrangements of the Glasgow Fever Hospital allow of a tolerably accurate estimate both of the usual mortality of typhus fever, and of the amount of stimulants habitually administered. Finally, it is very satisfactory to myself that this can be done without involving personal considerations, or placing my own practice in opposition to that of any other individual physician. For all these reasons, I propose, in the present paper, to confine the argument rigidly within the bounds assigned to it in the title, although it would be easy to strengthen my case by allusions to other diseases, and to other fields of practice than the hospital.

I expect, then, to be able to show in this paper—1st, That it is possible to reduce the mortality of typhus fever, while withholding a large proportion of the amount of alcoholic stimulants usually given; 2nd, That this diminution of mortality may take place at all ages, but is most marked among the young; 3rd, That while at all ages the administration of stimulants ought to be very strictly guarded, as likely to prove injurious when in excess, it is demonstrable that young and temperate persons may be advantageously treated (*i.e.*, treated with a diminished mortality) *without one drop of wine or spirits being given from beginning to end of the fever, except in the rarest casualties*; 4th, That the principle of giving stimulants *as a matter of routine*, in typhus (*i.e.*, at a certain stage of the disease, with but little regard to individual peculiarities), ought to be at once abandoned; 5th, That an approximation can be

made to a more correct doctrine on the subject, though further researches are still required; 6th, That there is reason to think that in most hospitals, as well as in private practice, a very large needless expenditure is incurred through neglect of these facts—a consideration which, though secondary in importance to others, is by no means to be set aside as irrelevant.

The facts from which I propose to attempt to prove these positions are—1. The cases treated by myself, or under my immediate superintendence, in the Glasgow Fever Hospital, since the middle of November, 1862; 2. The entire aggregate of cases treated in the years 1861 and 1862, these years being selected merely as being the nearest in point of time to my own appointment as physician, though only to a very slight extent (in the end of 1862) influenced by my own practice, and therefore in all probability fairly representative of the existing state of opinion and practice in Glasgow. I will add, that nothing in my experience leads me to believe that the practice in Glasgow is materially different from that usual in Edinburgh, or in Scotland generally, in this respect; while I am fully convinced that the tendency to what I regard as an excess is far greater in London, and probably in many parts of England, than it is in either Edinburgh or Glasgow.

The cases treated by myself are roughly divided into three series in the order of time. Although the numbers in these series are probably too small, separately considered, to give a secure basis for numerical analysis, I have thought it desirable to preserve the distinction, partly because it illustrates a very remarkable increase in the mortality of the fever in the middle period (April to June, 1863), and partly because the returns were carefully completed in private memoranda left me by my dear friend and fellow-labourer the late Dr. Hamilton, whose early death, from this very disease, typhus fever, has saddened all who knew him, as with the sense of an irreparable loss. To his earnest love of his profession, to his simplicity and truthfulness, not less than to his great quickness in receiving and applying new ideas without prejudice, I am more indebted than I can possibly express; and without his efficient assistance it is very certain that the facts in this paper could not have been presented in a satisfactory form. The basis thus obtained was

further extended, and even improved upon, by his successor and friend, Dr. Marshall, my assistant in the wards from August to December, 1863. To Dr. Duncan, my present assistant, I am also indebted for important aid in consulting the journals of the house; and I am entirely convinced he has carried out the analysis of these facts in a spirit of the most rigid impartiality, and with the most perfect accuracy that was possible under the circumstances.

The returns thus prepared have been reduced to a commodious form in several tables, now in the hands of members, on which I will proceed to make a few remarks.

The first table exhibits the result of 595 cases of typhus fever occurring in regular succession between November, 1862, and December, 1863, with the exception of a few cases admitted in June and July, which Dr. Hamilton had not included in his return, as they occurred after he had made it up at my request, and before Dr. Marshall took his place. I have not thought it necessary to re-open the documents in order to include these cases, as I feel certain they would in no way have modified the conclusions to be drawn from the rest. The utmost care has been taken to include every case, even when the patient has died within an hour or two after admission, and before any treatment could be brought into operation. Every case recorded in the journals as typhus was the subject of a special diagnosis, either on the ground of a characteristic eruption, or of such a marked association with a group of eruptive cases as left no reasonable doubt of the nature of the disease. In point of fact, a very large proportion, indeed, of the cases were distinctly *eruptive* typhus, the absence of eruption having been quite an uncommon fact in my later experience of typhus. The experience of my colleagues may be confidently appealed to, moreover, to show that there has been very little chance, during the present epidemic, of mistaking typhus for any other fever. The character of the epidemic has been exceedingly well marked, and no practical difficulty has arisen in classifying the cases. Nor is there any reason to suppose that the principles of diagnosis in 1861-62 differed materially from those employed by myself in 1862-63. The classification of the

cases in Table II. contains ample proof of the great predominance of typhus over all the other forms of fever admitted in these years.

The aggregate mortality of the 595 cases referred to as admitted under my care was 71, or within a fraction of 12 per cent. (See Table I.) It is remarkable that for about two months (constituting the second series) the mortality rose to 19 per cent.; and I ascertained at the time that this rise in the rate of death was experienced all over the fever wards, as well as in the cases under my own care, being quite evidently dependant upon a graver character of the cases admitted.

The mortality was, as usual, much greater among the adult and aged patients than among the young. Taking the age of 15 or 16 as the boundary line, it will be observed that among 406 cases above that age 70 died, or about 17·2 per cent.; while of the 189 cases below that age only *one* died, a child, aged 6 years, admitted only to die. (See Table I., note.) It is to be remarked, however, that while Dr. Hamilton included the sixteenth year (*i.e.*, 15-16) in his returns, Dr. Marshall excluded it in the third series, which is to this extent, therefore, different from the others. It did not appear to me that the difference in this respect was of very material consequence as to the result; but it remains to be stated that had Dr. Marshall included the sixteenth year there would have been one more death to record in upwards of 190 cases. The total mortality, therefore, of the cases decidedly below puberty (even including the infant admitted moribund) is less than 1 per cent., and might be reduced to absolutely *nil* by excluding the deaths within 24 hours, according to the practice of many hospitals.

Comparing now these results with those of 1861 and 1862 (Table III.), we find that the mortality per cent. of all ages in typhus is stated as 18·86 and 16·92, the average of both years together being 17·69 per cent.; while the adults died in the proportion of about 24 per cent., and the *young* in the proportion of 3·2 per cent. in the former, and 3·6 per cent. in the latter year. It need hardly be insisted on that the returns in Table I. present an advantage in point of mortality over those

in Table III., at every period of life. Thus, for a mortality at all ages of under 12 per cent. in Table I., there is, in Table III., $17\frac{1}{2}$ per cent.; for a mortality among adults of 17 per cent. in Table I., there is 24 per cent. in Table III.; and for a mortality of upwards of 3 per cent. among the young in the latter table, the former gives only one death of a child admitted moribund; and one other of a girl above 15 years of age, in a total of 190 cases or more. So far as these facts, therefore, can be admitted to be in any sense conclusive, they establish the position of a relatively low rate of mortality in the cases in Table I., both for the earlier and for the later ages.*

Again, comparing the results in Table I. with those given in Dr. Murchison's tables, deduced from the large experience of the London Fever Hospital over $14\frac{1}{2}$ years, we find that in place of a mortality at the rate of less than 12 per cent. for all ages, the average rate in London has been no less than 20·89 per cent. I am, indeed, very far from wishing to indicate that this comparison is really a fair one, for I know well how much the mortality of fever cases in different hospitals is due to other causes than differences of management; and I believe that the London Fever Hospital does in fact receive a larger proportion of old and destitute persons than most others; but Dr. Murchison has carefully compiled statistics from a number of different places, Glasgow included, from which he arrives at the conclusion that the average mortality of typhus fever in hospitals, excluding local and accidental causes of difference, is not less than 18 per cent.† In the practice of the late Dr. Todd, in King's College Hospital, Dr. Murchison found the general mortality of typhus to have been about 25 per cent.; and in two only of the 108 cases on which this deduction is founded did the death take place within 48 hours after the patient's admission, showing that the circumstances of the removal to hospital were not, in even so great a degree as usual, responsible for this high rate of mortality.

The preceding results may be briefly summed up thus:—

* The number of males and females in my wards being very nearly equal, I have not thought it necessary to make any subdivision of the returns according to sex.

† Treatise on Continued Fever, 1862, p. 218.

Rate of Mortality of 595 Cases in Table I.,		
without selection and without exclusion, -	11.93	per cent.
Average rate in London Fever Hospital,* -	20.89	"
The same, with exclusion of cases dying in		
less than 48 hours,* - - -	17.94	"
Rates in Edinburgh epidemic* of 1847-8,		
20.6 per cent. - - - - to	24.72	"
In Barony Parish Hospital, Glasgow, in the		
same epidemic,* - - -	17.23	"
In Glasgow Infirmary, 1843-53,* - -	18.00	"
In the same, 1857-61,* - - -	16.83	"
In the same, 1861 (Table III.), - -	18.86	"
In the same, 1862 (Table III.), - -	16.92	"
In King's College Hospital, under Dr. Todd		
(approximative),† - - -	25.00	"

Taking the cases of the young alone, as opposed to the adult population :—

Rate of Mortality of 189 unselected cases in		
Table I. (inappreciably small but say), -	1.0	per cent.
Rate in Glasgow Fever Hospital, 1861, -	3.2	"
Do. do. 1862, -	3.6	"
Do. do. 1847,‡ -	5.0	"
Rate in London Fever Hospital during 10		
years,§ - - - -	6.2	"
Rate in King's College Hospital under Dr.		
Todd, possibly about - - -	17.0	"

On the other hand, taking the adults alone, the differences

* Murchison, p. 217, 18.

† British and Foreign Med. Chir. Review, October 1860, p. 332. Read also the controversy in the Lancet, October and November, 1860.

‡ 163 Cases below 15 years of age, 3 deaths. Dr. Steele, in Edin. Med. and Surg. Journal, vol. 70, 1848.

§ 563 Cases below 15 years of age, 35 deaths. Murchison, p. 22, Table XV.

|| See data in British and Foreign Med. Chir. Review, as above. The numbers, indeed, are too small to justify a precise statement of the rate of mortality, but it is stated by Dr. Murchison that of 34 cases below 20 years of age, 6 died, or at the rate of 17.64 per cent. From the table in p. 221 of Dr. Murchison's book, it further appears that the rate of mortality in Typhus below the age of 15 is in London even higher than that of all the cases below 20. I wish, however, these statements to be taken as approximative only.

in the rates of mortality are, of course, much less striking, but are still in favour of Table I., which gives an average rate of 17·2 per cent., as compared with 24 per cent. in the Glasgow Fever Hospital in 1861-62.

I have now to refer to Table IV., in evidence that these very favourable results have been attained with a much smaller amount of stimulation than has been usual of late in Glasgow, or even in my own previous practice in the Edinburgh Infirmary. The figures in the table, indeed, speak so plainly upon this point that it would scarcely be necessary to make any remarks upon them, were it not that the subject is one of such vast importance, and that all numerical data, unexplained, are liable to a certain extent to misconstruction. I desire, then, that it should be very clearly understood that there is no evidence whatever in these numerical data of anything that can be called special profusion in the use of alcoholic stimulants in Glasgow, as compared with other places. On the contrary, I have a strong conviction, based upon facts which, though not capable of being numerically stated, leave me in little doubt, personally, that both in London and in Edinburgh (but especially in the former), the habitual administration of alcoholic stimulants in hospital practice has been at a higher rate than in Glasgow; and I am also well assured from detailed examination of the facts, that my own rate of administration in Edinburgh, even when at the highest, was decidedly less than that of some of my colleagues. On the other hand, the comparative moderation of the Glasgow annual averages leaves room for considerable individual varieties of practice; and here, too, it is quite plain to me that the excesses of some physicians are compensated in a great degree by the moderation of others.

With these explanatory remarks, I would now direct special attention to the averages of the third series of my own cases, as recorded in Table IV. It is possible, indeed, that this average may in the end turn out to be practically *too low* to meet the requirements of the severest types of epidemic typhus; but I am very sure that the cases to which these numbers apply were of quite average severity, according to my previous experience of typhus fever. It is true that during several years in Edinburgh, in which my numerical averages show a greater con-

sumption of alcoholic liquors, the fever was much milder, and even less fatal than in Glasgow in 1862-63; but I have now a most assured conviction, founded on careful observation of details which can hardly be stated here, but have been referred to elsewhere,* that the fever of that period was quite *exceptionally* mild; and I am now equally convinced that to whatever extent I gave stimulants greatly in excess of my present practice at the period referred to, they were misapplied and wasted, if not indeed in part ordered and used without my knowledge of the fact.† At all events, it is quite certain that in typhus fever by no means exceptionally mild, treated with a daily average of spirits not much more than half, and of wine less than the eleventh part of the quantity administered over the entire fever hospital in 1861-62, a rate of mortality was attained of not more than 10 per cent. Or, to put the result into the form of larger figures, the facts are as follows:—Between August 3rd and December 7th, 1863, there were treated 135 males and 134 females affected with typhus fever, being a total of 269 cases treated to a termination in my wards. Of these cases 27 died, being at the rate of 10 per cent. During the whole course of the treatment the 269 cases consumed 633 oz. of Port wine and 666 oz. of whisky, or at the rate of about $2\frac{1}{4}$ oz. of wine and $2\frac{1}{2}$ oz. of spirits on an average to each patient, as the allowance for his entire period of residence, which, on an average, was $20\frac{1}{2}$ days.‡

On the other hand, in the Glasgow Fever Hospital, during the years 1861 and 1862 taken together, there were 1837 cases under treatment, although only 1694 of these were treated to a termination during the period assigned, and only 1289 were typhus. There were consumed in the two years, 62,754 oz. of wine,

* Clinical Medicine, 1863, p. 153.

† The reader of pp. 58-63 of *Clin. Med.* will have no difficulty in perceiving that a suspicion of this kind was present to my mind in writing a former paper upon this subject, and the renewed experience in Glasgow, of a practice conducted upon nearly the same principles, but much more carefully watched in detail, convinces me that the apparent rise in the averages of alcoholic liquors from 1856 to 1860 was really due to a diminished personal control of the wine roll, rather than to an increased faith in the remedy.

‡ It is possible that the short period of residence was due in part to the great pressure in the Fever Wards during some part of the Autumn and Winter Months.

8,440 oz. of whisky, and 2,611 of brandy. Adding the two last sums together and dividing by the number of patients, we find that each patient consumed more than 34 oz. of wine and 6 oz. of spirits. But as much the largest proportion of this quantity was probably given to the 1289 cases of typhus, it is probable that the real average quantity consumed by the typhus cases may have been not less than 40 ounces of wine and 7 of spirits, which would be nearly 16 times as much wine and 3 times as much spirits as was given to the cases in the 3rd series above referred to. With this much larger consumption of alcoholic stimulants, the mortality of typhus was no less than $17\frac{1}{2}$ per cent. in these two years, while under the lesser amount of stimulants it was only 10 per cent.

It is plainly inexpedient to attempt to establish any fixed rule for the administration of stimulants upon a numerical basis, and it may be safely asserted that such a rule, if established by authority, would be set up only to be broken. But it is a consideration of no small importance, that if it should appear hereafter that typhus can be usually treated with an amount of stimulants, nearly such as I have here indicated, the saving to our hospitals would be about a bottle and a-half of wine, and more than three ounces of spirits, in each case of typhus fever. I do not wish, however, to press this view of the subject, which is plainly of a subordinate importance to the question of a sound and successful treatment.

All that I shall contend for at present, therefore, in regard to the mortality of typhus under a liberal use of stimulants, is that a fair presumption has been established in favour of a considerable reduction in the quantities at present habitually employed. In other words, I hold it as proved that such a reduction may be effected without any detriment to the safety and success of the treatment, and further, I regard it as very probable, that the general mortality of typhus will be lessened exactly in proportion as the routine of a vicious and excessive stimulation is abandoned in favour of a judicious and careful system of treatment by all the other means known to physicians. But in regard to the case of young persons, and especially persons below the age of puberty, when affected with typhus, I think the facts before me warrant a much stronger

and firmer position than this. I have long been convinced, in fact, by carefully watched experience of individual cases, that in persons of immature age, and of temperate habits, stimulants should very rarely be administered, except in minute and experimental doses, rather as tonics than as stimulants in the proper sense of the term; and possibly in some very extreme cases of collapse, in more considerable quantities; but not even then with any approach to intoxicating doses. In typhus fever, I am now well assured that this view is well founded, for I have to record the fact, that the treatment which gave only one fatal case in 189 young persons, was a treatment from which stimulants were practically excluded, the only patient who had a little wine being a child with *cancrum oris*, with great debility of the stomach, long after the fever had run its course. Looking at the results of this practice beside some of those formerly indicated, and especially looking to the regular rise in the rate of mortality among the young in typhus fever, in proportion as there seems reason to believe that the practice has leaned to a routine of stimulation, I confess I am strongly persuaded that *to the young, in typhus, and very probably in most other fevers, stimulants are not less than actively poisonous and destructive*, unless administered with the most extreme caution, and in the most special and critical circumstances. Had my 189 cases of young persons died at the rate even of those in the Glasgow Fever Hospital in 1861 and 1862, the deaths, instead of one, would have been six or seven; had they died at the rate of the Glasgow epidemic of 1847, which, however, I admit to have been a more severe epidemic than the present, the mortality would not have been less than nine; at the rate of the London Fever hospital, it would have been nearly twelve, while, in the hands of Dr. Todd, under a routine of such extreme stimulation as is indicated in his book on acute diseases, it seems probable that instead of *one death* in the 189 cases, there must have been no fewer than thirty to thirty-five!

I trust it will not be supposed from anything I have written above, that the views which I wish to suggest have anything in common with those of the *depletion and starvation* school of physicians, or such as condemn the use of stimulants, *in toto*, in fevers, from the idea that they tend to "excite the circulation." With this school of physic neither my own personal opinions,

nor my actual practice, have anything in common. I do not deprecate the excessive use of stimulants on account of their tendency to "excite the circulation," nor do I advocate the use of depletion or of starvation as a corollary to the doctrine of lessened stimulation; on the contrary, the whole aim of the practice I should advocate here, were I at liberty to extend the field of this discussion, is what is commonly called "supporting the strength;" and the objection that I entertain to the use of stimulants in excess, or upon the plan systematically advocated by the late Dr. Todd, is, that so given, they do not support the strength, but rather poison the system, by loading the blood with material which is inconvertible into blood or tissue, and which arrests or interferes with the vital changes necessary to preserve the balance of waste and supply. The essential questions, however, connected with this subject, are foreign to the plan of the present communication, and I only advert to them in order to draw a sharp line of distinction between the objections I entertain to excessive stimulation in fevers, and the practice so common a quarter of a century ago, of encountering the febrile excitement by depressing remedies, and by starvation or extreme restriction of the diet. I can well believe, therefore, that Dr. Alison, and after him Drs. Graves and Stokes, in introducing a "supporting" practice in fevers, in opposition to the depleting practice formerly followed, initiated a great improvement, and saved the lives of a great many of their patients. The only question with me is how to secure the advantages of a "supporting" practice, without at the same time, running a risk of poisoning the blood with repeated doses of alcohol, given without due observation of the effect of each dose, and so as to interfere with the real nutrition of the system, rather than to promote it. That all routine methods of administration tend to this abuse, I have satisfied myself by careful observation both of my own practice and that of others whom I have met in consultation and otherwise; and believing, as I do, that such a tendency to excess exists, I venture to ask the assistance of my medical brethren towards a fair and calm consideration of this great question, with a view to the great object of all our efforts in this society—the improvement of medical practice.

TABLES

IN ILLUSTRATION OF THE PRECEDING PAPER.

I.—Abstract of Cases, Deaths, and Mortality per cent. in Typhus, in cases treated by Dr. Gairdner, in Glasgow Fever Hospital, 1862-63.

CASES.

	All Ages.		Young.		Adult.
1st Series, -	225	-	72	-	153
2nd Do. -	101	-	31	-	70
3rd Do. -	269	-	86	-	183
	<hr/>		<hr/>		<hr/>
All three,	595		189		406

DEATHS.

	All Ages.		Young.		Adult.
1st Series, -	25	-	0	-	25
2nd Do. -	19	-	0	-	19
3rd Do. -	27	-	1*	-	26
	<hr/>		<hr/>		<hr/>
All three,	71		1		70

MORTALITY PER CENT. (APPROXIMATIVE.)

	All Ages.		Young.		Adult.
1st Series, -	11.1	-	—	-	16.3
2nd Do. -	19.0	-	—	-	27.1
3rd Do. -	10.0	-	?	-	14.3
	<hr/>		<hr/>		<hr/>
All three,	11.9		1?		17.2

* The one death was of a child aged 6, admitted moribund, and not seen by Dr. Gairdner, or placed under treatment. Another death however occurred during the 16th year, in a girl from Coatbridge; and since the return was made up as above, a death has occurred in a girl of six months, who had Typhus after Variola, and was admitted in a very low state.

II.—Abstract of Cases treated in Glasgow Fever Hospital, during the two years 1861 and 1862; shewing also the average period of residence (in days), and the total consumption of alcoholic stimulants (in ounces), during each year.

		1861.		1862.
Total under treatment,	-	790	-	1047
Treated to a termination,	-	709	-	985
Typhus,	- - -	509	-	780
Typhoid,	- - -	36	-	79
Variola,	- - -	73	-	43
Febricula,	- - -	54	-	29
Pneumonia,	- - -		-	18
Scarlatina,	- - -		-	10
Influenza,	- - -		-	11
Average period of residence,				
	(in days),	28	-	27
Total consumption of Whisky,				
	(in ounces),	3820	-	4620
Do.	Brandy,			
	(in ounces),	449	-	2162
Do.	Wine,			
	(in ounces),	29,272	-	33,482

III.—Abstract of Cases, Deaths, and Mortality per cent., in cases registered as Typhus (see Table II.,) in Glasgow Fever Hospital.

CASES.

	All Ages.		Young.		Adult.
Year 1861, -	509	-	154	-	355
“ 1862, -	780	-	247	-	533
	<hr/>		<hr/>		<hr/>
Two Years,	1289		401		888

DEATHS.

	All Ages.		Young.		Adult.
Year 1861, -	96	-	5	-	91
“ 1862, -	132	-	9	-	123
	<hr/>		<hr/>		<hr/>
Two Years,	228	-	14*	-	214

MORTALITY PER CENT.

	All Ages.		Young.		Adult.
Year 1861, -	18.86	-	3.2	-	25.6
“ 1862, -	16.92	-	3.6	-	23.1
	<hr/>		<hr/>		<hr/>
Two Years,	17.69		3.4		24.1

* The ages of these 14 fatal cases were as follows:—at 4 years, 1 case; at 6 years, 1 case; at 7 years, 2; at 9 years, 2; at 10 years, 1; at 11 years, 3; at 12 years, 1; at 13 years, 1; at 14 years, 2.

IV.—Averages of Wine and Spirits employed in Glasgow and Edinburgh Fever Wards, shewing the quantity (in ounces), ordered for each patient in twenty-four hours, at the periods, and under the circumstances mentioned below.

	Ounces per Patient ordered in twenty-four hours.	
	WINE.	SPIRITS.
Average of five years in Edinburgh, (Dr. G.,)...	1.314	0.137
Maximum—Average of year 1858, (Dr. G.,)...	1.734	0.346
Minimum—Average of year 1856, (Dr. G.,)....	0.715	0.069
1st and 2nd Series (Table I,) (Dr. G.,).....	0.261	0.214
3rd Series (Table I,) (Dr. G.,).....	0.115	0.121
Two years in Glasgow Fever Hospital (Table III,) (various physicians,).....	1.304	0.229

